a research on soap an ignoble and degrading occupation; as though atoms *per se* were inherently dignified and only became disreputable when associated with other atoms to form the molecules of a useful, if homely, commodity. There are many forms of snobbery. Pure science itself has had to put up with a good deal of classical snobbery, as Mr. Wells has more than once testified. I doubt whether matters will be mended by a development of such a form of scientific snobbery as seems often to be the inspiration of the disparagement, in academic circles, of applied science and industrial research.

Fortunately this attempt to erect an arbitrary and artificial barrier between pure and applied science is becoming progressively discredited as the nature of industrial research and its dependence on pure science are becoming better known. Every research in applied science, if it is to be thorough, involves some research in pure science. An industrial problem may be, and often is, the starting-point of a research that may widen the bounds of knowledge as much as any research born of a conception in pure science. Applied science and industrial research have been developed more widely in America than in any other country, if we except Germany. How does American experience confirm the view that to foster industrial research is to starve pure research? In the paper on "Industrial Research in the United States of America" by Mr. A. P. M. Fleming, published for the Department of Scientific and Industrial Research, there is abundant testimony to the recognition, by firms and institutions engaged in industrial research, of the importance of pure science research. Such an industrial leader as Dr. J. J. Carty, vice-president of the American Telephone and Telegraph Co., in his presidential address to the Institute of Electrical Engineers in 1916, emphasised this view: "By every means in our power, therefore, let us show our appreciation of pure science, and let us forward the work of the pure scientists, for they are the advance guard of civilisation. They point the way which we must follow." Mr. Elihu Root, chairman of the board of trustees of the Carnegie Institution of Washington, in a paper on the need for organisation in scientific research, makes the same point: "While the solution of specific industrial problems and the attainment of specific industrial objects will be of immense value, the whole system will dry up and fail unless research in pure science be *included within its scope*." Mr. W. A. Harmor, assistant director of the Mellon Institute of Industrial Research, University of Pittsburgh, bears similar testimony to the needs of pure science: "The wide view is now taken that, in considering the needs of industry, pure science investigation has as essential a contributory function as that specifically devoted to the attainment of some technologic objective." One could multiply almost indefinitely such tributes to the primary and paramount necessity of investigations in pure science from men and organisations concerned mainly with industrial research. Prof. Soddy's argument that for the million of money which the Government has expended or earmarked for scientific research pure science has got little or nothing is, therefore, based on a misconception of the nature of industrial research, and is directly contradicted by past experience and present knowledge.

The assumed antagonism between pure and applied science is baseless in fact and mischievous in tendency. As Mr. Harmor has well said: "Both pure and applied research are of the same order of importance and each has its own related field." The alleged inferior character of applied research as compared with pure research has no better foundation in fact than the alleged inferiority of scientific studies, as instruments of intellectual training, to classical studies. As Mr. A. W. Mellon, president of the Mellon National Bank of Pittsburgh, in an article on the value of industrial research, aptly expressed the matter: "The fundamental differences between pure research and industrial research are, indeed, traceable to the differences in the poise and personality of the representatives of each type of scientific investigation. Success in genuine industrial research presupposes all the qualities which are applicable to success in pure science, and, in addition, other qualities, executive and personal, more or less unessential in the pure research laboratory."

It would be strange if it were not in line with other experience that every time an attempt is made to extend and foster applied science and industrial research someone raises the cry that pure science is thereby being neglected and starved. This is to argue as though the total fund, both of money and energy, available for the purposes of scientific education and research were a fixed fund, so limited that any amount devoted mainly to the purposes of applied science must thereby lessen the sum available for pure science. It is a fallacy on a par with the trade-union notion that increased production by the individual worker will increase unemployment, and, by augmenting the profits of the employers, diminish the wages of the employees; and it is a proof, if proof were needed, that academic trade unionism can be as selfish and short-sighted as any other kind. Yet, just as the present condition of Europe affords a plain proof of the economic truth that the weakness of one nation impairs the strength of all, so will the cause of pure science not be bettered, but rather worsened, by attempts to crab the progress of industrial research.

The Department of Scientific and Industrial Research, as Sir Frank Heath has well said, is engaged on a great adventure. Thanks largely to its efforts, already the spirit of science is stirring among the dry bones of industries to which it was previously little known. The research associations formed and to be formed, which will cover a wide and diversified area of British industry, are attracting, and are destined to attract, scientific workers of the highest distinction and widest outlook, among them, no doubt, many of the members of the National Union of Scientific Workers, under whose auspices Prof. Soddy made his attack on the Department and the research associations. I submit that the cause of pure science is not well served by inconsiderate attacks on this industrial research movement, which is admittedly a novel experiment, beset by unforeseen, because unprecedented, difficulties, but the success of which must react to benefit pure science as well as to redeem British industry. J. W. WILLIAMSON.

26 Russell Square, W.C.1, May 6.

A Rainbow Inside Out.

IN February last Mr. P. H. Hepburn directed my attention to some surprising light-bows he had observed on several occasions on the surface of one of the ponds on Hampstead Heath. On February 24 we examined them together. A footpath lighted by three electric street-lamps runs along the southern edge of the pond. As one passed along this path bright bows of strange forms cast by the lamps were seen to spring out from the edge. The night was dark, the air still and slightly foggy, and the water smooth and covered with a film of scum extending as far as we could see in the dim light. There was no frost. The bows were judged to be on the surface of the water, which was only about a foot below the level of our feet.

The form of the bows is shown in Fig. 1, which is taken from a rough sketch made by the writer from memory after returning home and before any explanation of the phenomenon had suggested itself. Here O is the observer in three positions, I., II., and III., and the bows seen from these positions are numbered correspondingly. The twin bows seen in position I. and the double curvatures at α in positions II. and III. struck us as remarkable. As the observer moved from the first position to the second, the nearer branch of I. sank into the bank of the pond and the further branch sprang out in a somewhat startling manner until it attained the size roughly shown at II. The observer proceeding towards III., the apex of the bow, became lost in the distance.

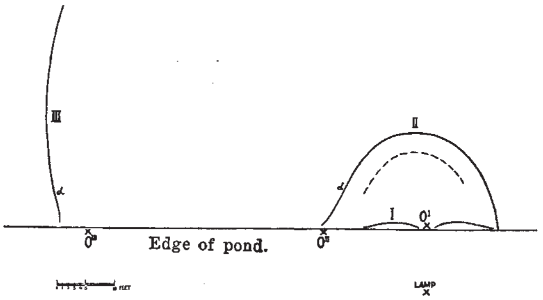

FIG. 1.—O^{I}, O^{II}, O^{III}, three positions of the observer's eye; I., II., III., the bows seen in these positions; α, points of double curvature.

The bows were colourless except for a tinge of red on the inside and of light of shorter wave-length on the outside. They formed the limiting inner edge of faint, diffuse luminosity extending over the general surface of the pond, and within the bows was darkness. In position II. a ghost of an inner bow was seen within the principal bow, roughly as shown in the figure. No colour could be distinguished in this inner bow.

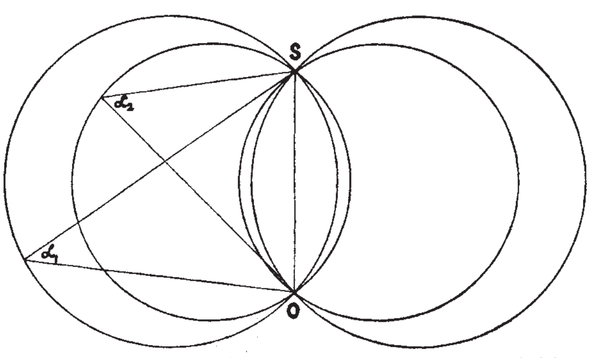

FIG. 2.—S, source of light; O, eye of observer; α_1,α_2, angles of minimum deviation for primary and secondary bows respectively.

In the first position the surface of the water was only about 5 ft. below the eye, and around the shadow of the observer's head—on the fog as we thought at the time, but on the water-surface as we now believe—the diffuse light was brighter, forming an aureole a few inches in breadth. When the head was turned slightly, so that a ray of light from the lamp to the water could pass close to the eye, the aureole at the edge of the shadow near this eye became brighter, suggesting that a ray reflected directly back on itself was of considerable intensity.

On subsequent nights we returned to the spot in the hope of again seeing the phenomenon, but, doubtless owing to a change in the weather conditions, we saw nothing. We took measurements, however, and from these data Figs. 3 and 4 were calculated.

The explanation of the phenomenon appears to be as follows:—In certain weather conditions globules of water are deposited from the fog upon the scum on the surface of the water, and the bows are formed in a way similar to those cast by the sun upon a bedewed field—with this difference: that here the source of light is near the observer. The bows, indeed, are rainbows.

In order that a drop of water shall be so placed as to return to the eye a maximum amount of light in the manner that obtains in the rainbow, it is necessary that the line joining the source to the eye shall subtend an angle α at the drop, where α is the angle of minimum deviation. The locus of such suitable positions for the drop is the surface of revolution of a circle, of which the line joining the source to the eye is a chord, about this chord (see Fig. 2). The locus, then, is a toroidal figure like a flattened apple. The locus for the secondary bow is such a toroidal figure within that for the primary bow. When the source of light is at infinity, as in the case of the sun, these figures become resolved into two co-axial conical surfaces having the eye at their common apex. In the present case the drops were confined to the plane surface of the water, and the bows seen were plane sections of these figures.

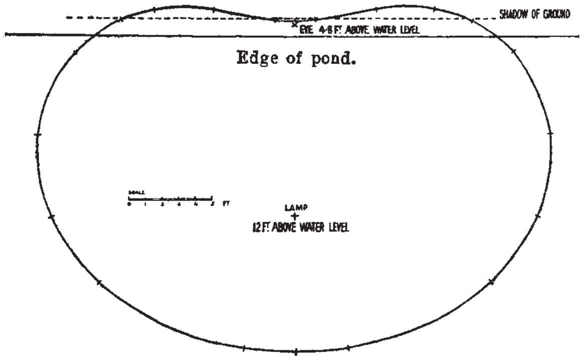

FIG. 3.—Primary bow, position I., as calculated.

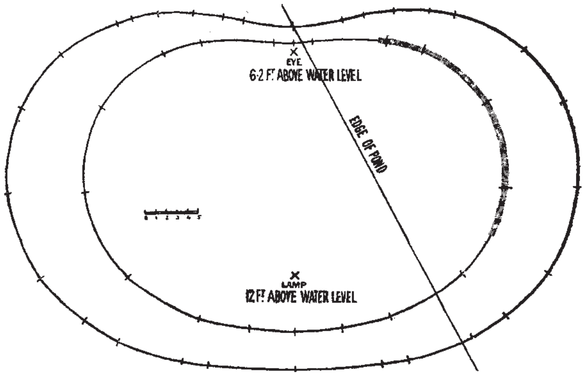

FIG. 4.—Primary and secondary bows, position II., as calculated.

In order to test the soundness of this explanation, the writer has calculated the form of the bows from the equation of the section of the toroidal figure made by the water-surface, using as data the measurements taken of the positions I., II., and III., and giving α the values 42° and 53° for the primary and secondary bows respectively. The results for I. and II. are shown in Figs. 3 and 4. It is unquestionable that these curves faithfully represent the phenomenon as we observed it, the portions shown with thicker lines being those within the limits of the water, which alone we were able to see. The calculated curve for III. (not shown) is equally corroborative. The aureole seen round the shadow of the observer's head is consistent with the attribution of the phenomenon to water globules upon a surface (see J. M. Pernter, "Meteorologische Optik," 1910, p. 424).

It is of interest to note that the closed curve is a rainbow inside out, and with the secondary bow within the primary.

Since writing the above we once again found the bows visible, and a careful examination of them seemed to confirm the conclusions arrived at on every point. On this occasion the film was broken by spaces of clear water, and at these spaces the bows were interrupted. The space within the inner bow was filled with faint, diffuse luminosity, and a marked feature was the blackness of the zone between the inner and outer bows.

C. O. BARTRUM.

32 Willoughby Road, Hampstead.

"All-or-None" in the Auditory Nerve.

PROF. D. C. MILLER ("The Science of Musical Sounds," 1916, p. 184) admits the reality of beat-tones, but says that they are purely subjective, having no physical existence. This seems unsatisfactory. To begin with *beats*, it is wrong to say that it is the places of maximum intensity which are properly called *beats*. This is an illusion, due to too familiar diagrams. The maximum of intensity occurs at no place, but at a point of time which, at its own instant, the maximum not being absolute, is not impressive. At any point of time the next vibration of a sound may be of greater amplitude or it may not, and the listening ear, being unable to foretell, cannot tell us in the present when the maximum is attained. The perception of a maximum is bound to arrive, in fact, the day after the fair, when the sound is on the wane. On the contrary, it is the minimum of intensity which gives the effect of the beat. This is clear if the two primary tones are of equal amplitude and there is a phase of silence, when the difference of sensation is a difference, not of *degree*, but of *kind*. It has been shown that if a musical note is suddenly reduced to silence, the interruption of the series of vibrations restores the last of the series of periodic impulses to its isolated value; the note ends with a kind of shock or tap, comparable to one of a series of hard beats. If periodic beats are rapid enough, the final impulses at the interruptions form a fresh series, and are free to evoke in the sensorium a sensation of tone of the same frequency as the beats, a beat-tone; and this is best observed when the beats are not too violent.

Beat-tones are, therefore, no more subjective and have no less physical existence—although they may have been invisible hitherto in tracings and photographs—than any other real tone; and since both beat-tones, p-q and $2q$-p, are best heard, at least with intervals less than an octave, when the primaries are not powerful, there never has been a good reason for rejecting Young's view of their origin, nor for ascribing to Kœnig the discovery of those "upper" beat-tones which were discussed by Young before the Royal Society in 1800. (In Faraday's copy of the 1807 quarto there is a book-mark at p. 544 of vol. ii., perhaps indicating that the chapter on the coalescence of musical sounds has more than an historical interest.)

But beats are produced by primaries of unequal amplitude, and in such cases there is no phase of silence, and apparently no absolute minimum of intensity. Here we have something comparable to the first *d* in "would do" rather than the first *t* in "not too," to a *voiced* rather than a *voiceless* occlusive. If in the physiology of hearing we assume similarity of character in the nervous impulse to that which is established for motor nerves, the contribution of a single nerve-fibre, not being greater than the faintest sound audible in a sound-proof room, is in ordinary circumstances imperceptible; in the fluctuating intensity of a beating note many fibres will be implicated at the maximum; at the minimum, relatively few. At a minimum of intensity which is not absolute some fibres will continue to conduct the series of impulses of the note, while others will at this instant discontinue, and the impulse preceding the interruption may evoke the displeasing sensation of a noise, whereas with slow beats, where the discontinuity in many fibres is spread over a longer time, the effect is not so, but pleasant. Hence with two primaries of nearly the same frequency we may hear at the same time beats and the beating note. When the interval between the primaries is sufficient for the frequency of the beats to be that of a real tone, we may in the same manner hear at the same time the separate primaries and either the beats or the beat-tone, or both the beats and the beat-tone. Further application of this principle will be found to offer a solution of other obscure problems in hearing.

W. PERRETT.

University College, Gower Street, May 20.

British and Foreign Scientific Apparatus.

MY attention has been directed to a letter appearing in the issue of NATURE for May 6 dealing with the subject of scientific apparatus. Your correspondents are extremely moderate in tone, but they do not state the class of apparatus to which they are referring.

Members of the association of which I have the honour to be chairman manufacture a large number of scientific apparatus, not only in glass and porcelain, but also other goods as well. Some of these were manufacturers in this country before the war, and proved by the quality of their products that they were able to stand against foreign competition; other members have entered the scientific trade only since the outbreak of the war, mainly at the request of the Government. The difficulties they have experienced have been extremely great, but they can prove that the quality of most glassware articles turned out is equal in many respects to that of articles previously imported from abroad.

Certain complaints have reached us; these have been most carefully investigated, and in many cases we found that the complaint dealt with glassware which was not manufactured by our members, but had been sold without any mark or badge of the manufacturer. Our members will be only too pleased to co-operate in every possible way with scientific workers, and look to them for the help necessary in establishing this "key" industry and placing it on a thoroughly sound basis.

As regards State aid, we do not want this in the form suggested, but rather we desire the creation of some method by means of prohibition whereby the industry will be enabled to establish itself, and at the same time the customer will not be penalised by being unable to get his material or the quantity of apparatus he desires. Under the form of prohibition which has been suggested, all orders for apparatus from abroad would have to come before a Special Committee of the Board of Trade. This Committee would